2012-04-23

Career Effects of Occupation-Related Vocational Education: Evidence From the Military's Internal Labor Market

Pema, Elda

Elsevier Ltd.

Economics of Education Review 31 (2012) 680–693

Contents lists available at SciVerse ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Career effects of occupation-related vocational education: Evidence from the military's internal labor market

Elda Pema*, Stephen Mehay

Graduate School of Business and Public Policy, Naval Postgraduate School, Monterey, CA 93943, United States

ARTICLE INFO

Article history:
Received 3 November 2009
Received in revised form 12 April 2012
Accepted 23 April 2012

JEL classification:
I2
J24
J45
M51
M53

Keywords:
Vocational education
Turnover
Promotion
Internal labor markets

ABSTRACT

Prior research on the labor market success of secondary vocational education has produced mixed results, with several studies finding wage gains only for individuals who work in training-related occupations. We contribute to this debate by focusing on a single occupation and organization and by comparing the careers of employees with and without occupation-related training in high school. We use longitudinal data on the careers of military recruits who completed high school Junior Reserve Officers' Training Corps (JROTC), a military science program that has features of a vocational training and school-to-work program. We find that the occupation-specific training received via JROTC reduces early turnover and improves long-run job stability for those who choose military jobs, suggesting that an important effect of vocational training is to improve job match quality. We also find that promotion rates for vocational graduates are similar to their peers, suggesting that vocational education in general works by improving occupational sorting.

Published by Elsevier Ltd.

1. Introduction

The role of vocational education in the high school curriculum has long been a controversial topic in education reform debates (Levesque, Lauen, Teitelbaum, Alt, & Librera, 2000; U.S. Department of Education, 2004). The controversy is fueled in part by mixed results on the labor market effects of vocational education (for a survey see Bishop & Mane, 2004). Some studies report positive wage effects when vocational graduates work in training-related jobs (Neuman & Ziderman, 1991, 1999). Since only 43% of vocational graduates work in occupations that match their training (Bishop, 1989), this finding raises questions about the effectiveness of vocational education. Hotchkiss (1993) finds no short-run wage gains, regardless of whether vocational education matches future occupations. Meer (2007) finds that long-run wage gains for vocational graduates are due to students' self-selection into tracks (vocational or academic). These findings have divergent policy implications and highlight the need for a better understanding of the pathways via which secondary vocational education affects labor market success.

Our study analyzes the impact of vocational education using data for employees in one occupational category. Examining within-occupation outcomes avoids confounding the effects of vocational training with occupational self-selection. In addition, rather than focusing on wages as in prior studies, we examine early turnover, long-run job attachment, and productivity. This allows us to pinpoint the channels through which vocational education contributes to job market success. For example, vocational education may directly enhance job skills within an occupation, thus increasing worker productivity (human capital effect). Alternatively, vocational education may improve *ex ante* information about specific jobs, professions, and employers, resulting in more stable or longer careers (job

* Corresponding author. Tel.: +1 831 656 3631; fax: +1 831 656 3238.
E-mail address: epema@nps.edu (E. Pema).

0272-7757/$ – see front matter. Published by Elsevier Ltd.
http://dx.doi.org/10.1016/j.econedurev.2012.04.005

match effect). Wage gains could result in either case, but for policy purposes, it is important to know whether vocational education increases wages by enhancing productivity, or by increasing the job-market attachment. We investigate what drives the estimated wage gains in the vocational literature, after holding constant occupational selection, firm-specific training, and across-firm variation. Our findings also shed light on why the documented wage effects are more pronounced in the long run and for those working in occupations that match most closely their vocational training.

We focus on a high school military science program, Junior Reserve Officers' Training Corps (JROTC), which is similar to other vocational education programs in its scope and curriculum. However, unlike other vocational programs whose graduates enter different firms and occupations, JROTC prepares students for careers in the military. Exploiting this link, we use data on U.S. Navy recruits and observe the performance of JROTC graduates in their military careers. We analyze vocational training effects by comparing the performance of new 'hires' with and without JROTC training.

Military data are well suited for this analysis, since the military represents both an employer and a broad occupational 'cluster' (Levesque, Laird, Hensley, Choy, & Cataldi, 2008). The military's rigid personnel system holds constant factors that confound the estimated effects of vocational education when using public data, such as differences in the amount of firm-specific training or placement of employees in different or fast tracks. Most importantly, the military data allow us to examine the direct effect of vocational training on both job performance and on job attachment.

Another problem we can sidestep with these data is the definition of 'occupation' and identifying 'occupation–training matches.' Vocational programs feed into many different occupations and prior studies vary in their definitions of occupational categories.[1] Furthermore, the way occupational controls, broad or narrow, are factored in the estimations varies greatly, complicating the comparison of effects across studies. Including occupation controls may lead to an overestimation of vocational training effects if individuals self-select into various occupations based on their comparative advantage. While adjusting for occupational self-selection can yield causal effects, it leaves open the possibility that vocational training works precisely by improving such sorting, rather than by improving occupation-specific skills. Given our large sample of individuals working in the same occupation, we can avoid bias both from occupational self-selection and from across-occupation wage variance.

While the JROTC–military relationship is unique in that training is linked to one employer, the analysis provides insights that generalize across vocational programs and employers. JROTC mimics typical vocational training in its goals, curriculum, and target population. For example, JROTC enrollees typically are non-college-bound students interested in learning about a potential occupation. JROTC offers elective courses that impart skills used in the military. Similar to school-to-work programs (STW), JROTC conveys information about the profession by both simulating military life and by providing instruction from former military personnel.[2]

Although not a profit-maximizing firm, the US military is a cost-minimizing organization that competes for recruits by designing contracts that attract individuals with the requisite skills. Labor economists traditionally have analyzed military enlistment as an occupational choice with recruits weighing the benefits and costs of enlisting relative to civilian employment opportunities (see, e.g., Asch & Hosek, 2007; Warner & Asch, 2001). To be competitive in the youth labor market, the military must tailor compensation packages to attract and retain the required quantity and quality of personnel (Hosek & Sharp, 2001). Similar to private firms, the military offers firm-specific training, and the return to that training depends on the expected employment duration of training recipients. Due to the absence of lateral entry, the military seeks new recruits who stay in service sufficiently long to allow recoupment of training costs. These constraints force the military to define job match the same as private firms – in terms of low turnover (Jovanovic, 1979). While it may appear that the military can obligate recruits to binding contracts until the costs of recruiting and training are recovered, in reality the military does not gain from employing or retaining individuals who are poor matches. Therefore, about 30% of new recruits leave the military without completing their service obligations and without any repercussions. Thus, the military aims to improve the quality of match at entry, allows those who are mismatched to leave, and incentivizes continued employment via reenlistment bonuses, similar to other firms that use compensation packages to hire and retain employees.

2. Background

JROTC enrolls over 500,000 students in more than 3300 high schools (20% of all public high schools).[3] As in vocational and STW programs, JROTC offers both academic and vocational courses and is linked to a specific employer.[4] The curriculum includes core subjects such as citizenship, communications, geography, health, and physical fitness. Each high school's JROTC program is affiliated with one of the military branches and uses retired military personnel

[1] In general, the vocational education literature defines 'occupation' very broadly. Neuman and Ziderman (1999) identify eight occupational categories for vocational education, whereas Hotchkiss (1993) identifies only two.

[2] For an analysis of STW programs, see, e.g., Neumark and Rothstein (2006).

[3] For information on JROTC see Coumbe, Kotakis, and Gammell (2008) and Laurence and Estrada (2003).

[4] STW programs include school-based learning, work-based learning, and connecting activities. School-based learning includes academic and vocational courses; work-based learning includes hands-on job training, mentoring, and instruction in a workplace (via internships and apprenticeships). STW connection activities establish partnerships with employers to ease the school-to-work transition (Neumark, 2009). JROTC is comparable to STW activities such as job shadowing, mentoring, and internships.

from that branch as instructors.[5] In addition to general military courses – such as military history, national security issues, and leadership – each program offers branch-specific courses. For example, the Army JROTC curriculum includes geography, earth sciences, and orienteering; the Air Force curriculum includes aerospace and aerodynamics; and core classes in the Navy curriculum are naval science (including sea navigation, rules of the road, and shipboard life), oceanography, meteorology, and Navy tactics and strategy.[6]

Although it is similar to vocational education and STW programs, JROTC has been overlooked by education researchers. This oversight may stem from the perception that military science classes represent extracurricular activities that do not affect employment, a perception fostered by the U.S. Department of Education's classification of high school military science classes as 'enrichment/other' rather than vocational education (Levesque et al., 2000). This designation contradicts the Department of Education's own definition of career technical education as classes that teach "... skills required in specific occupations or occupational clusters" (Levesque et al., 2008, p. 3).[7] More important, this classification misrepresents the scope and content of JROTC.[8] The curriculum, the use of military instructors, and the close link with the employer are clear indicators of the program's vocational orientation.[9] Military science 'concentrators' (students with at least 3.0 Carnegie credits) receive an advanced pay grade if they enlist. About 40% of such concentrators enter the military (Taylor, 1999), which is similar to the 43% of vocational students who find jobs in training-related civilian occupations (Bishop, 1989).

Prior research on JROTC is limited. Elliott, Hanser, and Gilroy (2002) analyze a federal pilot program that combined career academies with JROTC.[10] They find that JROTC Partnership Academy students had better academic achievement than students in a general track or 'regular' JROTC students. Pema and Mehay (2009, 2010) investigate several in-school and post-school outcomes of JROTC. They find that JROTC students are far more likely to enlist than their peers. They also find no employment effects for JROTC students who do not enlist, suggesting that program effects may be confined to those who choose a training-related occupation.

Other relevant studies include those that investigate career interests and *ex ante* labor market knowledge. Neumann, Olitsky, and Robbins (2009) examine the wage effects of job 'congruence' and find positive effects when college graduates' career interests and values are congruent with their occupational work environment. Job match quality is based on the degree to which individuals' interests match their occupation's characteristics. Polachek and Robst (1998) find higher wages for workers with higher scores on the 'Knowledge of the World of Work' test, which measures the degree of occupational information. These studies suggest that STW and vocational programs work in part by aligning students' interests and by providing *ex ante* labor market information, which improves occupational sorting and job match quality.

3. Data

We use data on all recruits who entered the Navy between 1994 and 2001. We restrict the sample to those with four-year contracts and we track each new recruit for 5 years or until separation. The Defense Manpower Data Center provided the personnel data, which contained 367,241 observations. We delete those with prior military service to maintain homogeneity of initial skill training and career paths, leaving 325,560 observations. Recruits are identified as JROTC completers if they earned at least 3.0 Carnegie units in military science (the equivalent of an 'occupational concentrator') (Levesque et al., 2000; Levesque, 2003a, 2003b).

To determine whether vocational education works by increasing human capital or by improving sorting, we analyze alternate measures of career progression and job match quality. First, we investigate two measures of job match quality: (a) early turnover during the recruit's 4-year contract; and (b) voluntary reenlistment decisions at the end of the 4-year contract. Early turnover (called 'attrition' in military parlance) reflects job mismatch. All mismatches result in an individual being discharged regardless of whether the dissatisfaction originates with the individual or with the organization.[11]

Second, to assess long-term job stability we analyze reenlistment decisions. Early turnover and reenlistment are indicators of job match quality and job stability,

---

[5] Instructors are hired by local school districts who share instructor salaries with Department of Defense.

[6] The JROTC curriculum covers 180 h per year, or 1.0 Carnegie unit. Generally, 130 h are devoted to core subjects, while 50 h are taken in elective courses (Coumbe et al., 2008).

[7] The Department of Education (1987) labels military employment as a 'non-labor market activity,' which contradicts how economists analyze military manpower supply issues. In an all-volunteer military, employment in the military is a voluntary occupational choice made by youth who weigh the monetary and non-pecuniary attributes of available jobs. Like other employers, the military sets minimum standards for entry and must offer compensation packages sufficient to attract and retain the required quantity and quality of personnel (Asch & Hosek, 2007; Warner & Asch, 2001).

[8] Another inconsistency is that for secondary schools DoE defines an occupational category called 'protective services,' which omits military science classes, whereas for colleges the same 'protective services' vocational category includes military science classes (Levesque et al., 2008).

[9] Military pay is competitive for non-college-bound youth. The earnings of new recruits exceed the median earnings of comparable non-college high school graduates. In 2009 enlistees entering in grades E1–E3 earned the equivalent of $34,752–$37,803 in taxable earnings, excluding benefits (retrieved January 11, 2009 from www.defenselink.mil/militarypay/mpcales/calculator/RMC.aspx) as compared to median civilian earnings of $25,012 for a 20–24-year-old male (retrieved January 11, 2009 from www.bls.gov).

[10] The 'Federal–Local Partnership for Serving At-Risk Youth Program' was jointly sponsored by the Departments of Education and Defense and attempted to combine the strengths of JROTC with the career academy focus on work-based learning (Hanser & Robyn, 2000).

[11] Supervisors may discharge recruits for poor performance or behavioral problems, or recruits may provoke discharges by deliberately performing poorly or displaying behavioral problems. Klein et al. (1991) show that official separation codes seldom identify the true reason for a mismatch and Buddin (1984) points out conceptual difficulties in distinguishing military 'quits' from 'fires.'

respectively, which are problematic in youth labor markets (Yates, 2005). Neumark (2002) shows that unstable early job experiences of youth have lasting adverse effects on adult labor market outcomes.

Third, to investigate the human capital effect of vocational training, we analyze promotion outcomes. Most new recruits enter in grade E1, but some enter in higher grades if they have completed some college courses or a JROTC program. Those who enter in grade E1 normally advance rapidly to E3, since these steps are administratively awarded. In contrast, promotions to grades E4 and E5 are competitive and depend on demonstrated performance, supervisors' evaluations, and skill qualification exams (Williamson, 1999).[12] We use promotion to E4 and E5 as measures of job performance. It should be noted that, due to administrative wage setting, wages in the military are not connected to productivity except through promotions. Compared to promotions, however, military wages introduce more noise because they also depend on the local cost of living, number of dependents, and other similar administrative rules, which are not related to performance.

Table 1 provides descriptive statistics. Women and African Americans are disproportionately represented among JROTC recruits compared to other recruits. JROTC recruits have slightly lower AFQT scores (by 3 points), but this difference is explained largely by the racial composition of JROTC participants; controlling for race reduces the AFQT score gap to only 1.2 points. About 90% of JROTC recruits possess only a high school diploma, whereas the educational attainment of other recruits is more dispersed. This suggests that JROTC participation does not help high school dropouts enlist. It also suggests that JROTC participants tend to enter the military directly after high school without first exploring college opportunities. In terms of career outcomes, JROTC recruits have, on average, lower turnover, higher reenlistment rates, and lower promotion rates than other recruits.

Table 1
Descriptive statistics.

Variable	JROTC recruits	Non-JROTC recruits
Demographics		
Male	0.782	0.828
	(0.413)	(0.378)
Female	0.218	0.172
	(0.413)	(0.378)
Caucasian	0.514	0.627
	(0.500)	(0.484)
African-American	0.354	0.184
	(0.478)	(0.388)
Hispanic	0.078	0.108
	(0.269)	(0.310)
Asian	0.028	0.045
	(0.164)	(0.208)
Native American	0.017	0.027
	(0.128)	(0.162)
Other race	0.009	0.009
	(0.093)	(0.093)
Married, children	0.038	0.048
	(0.191)	(0.215)
Married, no children	0.002	0.001
	(0.040)	(0.035)
Education and ability		
No high school diploma	0.028	0.037
	(0.164)	(0.189)
Certificate or GED	0.045	0.072
	(0.208)	(0.259)
High school diploma	0.902	0.842
	(0.297)	(0.365)
Some college	0.018	0.031
	(0.132)	(0.173)
College degree	0.007	0.018
	(0.084)	(0.132)
AFQT percentile	57.582	60.768
	(18.318)	(18.794)
Outcomes		
12-Month turnover	0.172	0.210
	(0.377)	(0.407)
24-Month turnover	0.247	0.285
	(0.431)	(0.452)
36-Month turnover	0.312	0.342
	(0.463)	(0.474)
First-term (four year) turnover	0.353	0.387
	(0.478)	(0.487)
Reenlist	0.443	0.390
	(0.497)	(0.488)
Promote to E4	0.828	0.886
	(0.377)	(0.318)
Promote to E5	0.198	0.268
	(0.398)	(0.443)
Observations	9347	316,213

Notes: The sample includes Navy recruits who enlist during 1994–2001 with 4-year contracts. It excludes prior enlisted recruits.

4. Estimating the effect of JROTC on careers

We first propose the following model for estimating the effects of vocational education on turnover, reenlistment, and promotion:

$$y_i = 1(\delta JROTC_i + \beta_1 \mathbf{X}_i + \beta_2 \mathbf{A}_i + \beta_3 \mathbf{M}_i + u_i > 0), \quad i = 1, \ldots, N \qquad (1)$$

where y_i denotes the outcomes (turnover, reenlistment, promotion) for individual i, $\mathbf{X}_i$ includes demographics (race/ethnicity, gender, marital status, dependents, and an interaction of the last two variables), and $\mathbf{M}_i$ represents institution-specific variables, including eight cohort dummies and ten dummies for military specialty. After receiving initial (basic) training, most new recruits receive specialty training. Controlling for military specialty isolates the effect of vocational education from the effect of firm-sponsored training. The cohort dummies proxy for civilian labor market conditions and other unmeasured differences across cohorts (due, for example, to fluctuations in recruiting policies). $\mathbf{A}_i$ includes ability proxies based on AFQT scores and educational attainment.[13]

[12] The personnel system consists of nine grades, E1–E9: Grades E1–E3 represent trainee and apprentice positions; grades E4–E6 represent technician and work group manager/leader positions; and grades E7–E9 represent supervisory positions.

[13] Rodgers and Spriggs (2002) suggest that AFQT scores are not good proxies for ability because they depend on age and education, and that AFQT scores should be adjusted before introducing them in wage regressions. We do not adjust AFQT scores for age and education because in our sample, compared to most public datasets, the screening of military

We initially estimate Eq. (1) via probit, which assumes that u_i does not include unobservables correlated with program participation and outcomes. However, this approach ignores the possibility that JROTC participation is endogenous. For example, if individuals who participate in JROTC have a stronger taste for the military, they would be more likely to fulfill their contracts or to reenlist.[14] On the other hand, the correlation could be negative if JROTC attracts disadvantaged students (for evidence see Pema & Mehay, 2009). If JROTC recruits are negatively selected (compared to both other high school students and to other recruits), our baseline estimates may be negatively biased.[15] We offer two alternative methods to address the self-selection problem.

Our first way of dealing with selection bias treats program participation as endogenous. For recruit i living in area j we specify a bivariate probit model:

$$y_{ij} = 1\left(\delta JROTC_{ij} + \gamma_1 \mathbf{X}_i + \gamma_2 \mathbf{A}_i + \gamma_3 \mathbf{M}_i + \gamma_4 \mathbf{L}_j + u_{ij} > 0\right), \quad i = 1, \ldots, N, \quad j = 1, \ldots, J \tag{2}$$

$$JROTC_{ij} = 1(\pi_1 \mathbf{X}_i + \pi_2 \mathbf{A}_i + \pi_3 ZIPJROTC_j + \pi_4 \mathbf{L}_j + v_{ij} > 0) \tag{3}$$

$$\begin{bmatrix} u_{ij} \\ v_{ij} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\right) \tag{4}$$

We create an instrument by matching recruits' home addresses (zip code) with the addresses of high schools located in the same zip code that offered JROTC.[16] We instrument program participation with an indicator – $ZIPJROTC_j$ – for whether a high school in the recruit's home zip code offered JROTC. Eq. (3) assumes that program participation is a function of individual characteristics and ability ($\mathbf{X}_i$, $\mathbf{A}_i$), the presence of a school that offers JROTC in the zip code ($ZIPJROTC_j$), and local area characteristics ($\mathbf{L}_j$). Identification requires that the excluded variable, $ZIPJROTC_j$, predicts JROTC participation, but is not correlated with unobserved factors associated with job performance y_{ij}.

The first assumption for identification appears to be met since Pema and Mehay (2010) show that over 80% of high school students who complete JROTC do so if their school offers the program.[17] Also, in regressions of Eq. (3), described below, $ZIPJROTC_j$ always has a statistically significant coefficient. The second condition for a valid instrument is that $E[y_{ij}|JROTC_{ij}, \mathbf{X}_i, \mathbf{A}_i, \mathbf{M}_i, \mathbf{L}_j]$ should not depend on whether schools in a recruit's home zip code offered JROTC. Ideally, if schools randomly offered the program, $ZIPJROTC_j$ would not affect career outcomes other than via individual participation. Therefore, in the absence of school-level selection into JROTC, this model would adequately address individual self-selection. However, Pema and Mehay (2010) find that JROTC programs are often located in inner cities and areas with high recruiting potential. Consequently, program location may be correlated with other unobserved characteristics of the recruit and the area, both of which may affect career outcomes.

To deal with this problem, Eq. (3) explicitly controls for socioeconomic attributes of the recruit's community ($\mathbf{L}_j$). These variables include county-level unemployment rates and per capita earnings for each year from 1990 to 2007. These variables capture local economic conditions from the time recruits are in high school through the end of their first term of service (when they make reenlistment decisions). From the 1990 and 2000 Censuses, we also obtain other county-level characteristics, including the population in the armed forces and in poverty, and the percent of the population that is black, Hispanic, or American Indian. Since placement of JROTC units tends to focus on poorer areas and those with high recruiting potential, we expect that total, rather than percent, military and poverty population are better controls for local area characteristics correlated with the presence of a JROTC unit. Inclusion of the county-level variables allows the effect of our IV to emerge only via individual participation in JROTC. These county-level variables proxy for local socioeconomic conditions that affect a school's ability to qualify for JROTC and that also may be correlated with job match quality and military performance.

If there are other unobservable local area features that are correlated with JROTC participation and military performance, our bivariate probit results would be biased. For example, if areas with JROTC high schools also offer fewer employment opportunities, JROTC recruits may be more likely to stay in the military. Controlling for local unemployment, earnings, and poverty addresses this issue, but there still may be other unobservable local conditions that affect both program participation and job attachment. In addition, while the previous analysis controls for *county*-level factors that may reflect local labor market conditions, it may fail to capture fully the characteristics of the smaller neighborhoods (zip codes) where recruits resided.

To address these concerns, we also obtain program effects by comparing JROTC and non-JROTC recruits who resided in the same zip code. These fixed effects models net out both observable and unobservable characteristics at the zip code level that may be correlated with job outcomes and the presence of JROTC. If we enhance the original model to include the unobserved characteristics of the local area l_j we obtain:

$$y_{ij} = 1(\phi JROTC_i + \theta_1 \mathbf{X}_i + \theta_2 \mathbf{A}_i + \theta_3 \mathbf{M}_i + \theta_4 \mathbf{L}_j + l_j + e_{ij} > 0), \quad i = 1, \ldots, N, \quad j = 1, \ldots, J \tag{5}$$

applicants reduces heterogeneity along these dimensions. In addition, we are holding education and many other variables fixed.

14 Pema and Mehay (2009) show JROTC participants have stronger preferences for military careers than other high school students. In contrast, our sample includes only enlistees, which reduces the problem of taste selection. However, if JROTC recruits have relatively stronger tastes for the military than other recruits, job attachment estimates still may be biased upward.

15 Any negative selection into the program in high school is likely reduced by the screening of military applicants based on educational attainment and aptitude.

16 Zip codes for each recruit's home address were provided in the personnel files. Zip codes for each JROTC high school were obtained from each service's cadet command.

17 Some students take JROTC classes in nearby schools when classes are not available in their own school.

Assuming that e_{ij} follows a logistic distribution conditional on both observable and unobservable variables, Eq. (5) below can be estimated via conditional maximum likelihood:

$$P(y_{ij} = 1|\mathbf{X}_i, \mathbf{A}_i, \mathbf{M}_i, \mathbf{L}_j, JROTC_i, l_j) = \Lambda(\phi JROTC_i + \theta_1 \mathbf{X}_i + \theta_2 \mathbf{A}_i + \theta_3 \mathbf{M}_i + \theta_4 \mathbf{L}_j + l_j) \quad (6)$$

This approach provides estimates that are conditional upon both observable and unobservable area-specific effects $\mathbf{L}_j$ and l_j.[18]

5. Baseline estimates

Table 2, panel A presents probit estimates of the turnover and reenlistment models. We analyze turnover at 12-month intervals over the 4-year term of service. If job information acquired via JROTC improves career decision-making, we expect program participation to improve job match quality. We estimate separate program effects by gender because women experience higher turnover and lower retention in the military (Buddin, 2005). One potential explanation for this gender gap is that women have lower tastes for the military. Hence, by providing a realistic preview of military life, JROTC may help women assess potential military careers more than it helps men.

The probit results indicate that JROTC participants have lower early turnover than other recruits (by 3 percentage points). This effect is similar for males and females. The sample for the reenlistment model in panel B of Table 2 includes only recruits who survive the first 36 months of service (turnover averages about 35% during the first 36 months). JROTC participation appears to increase reenlistment by 5.5 percentage points (9%).[19] If individuals who survive the first 36 months have stronger tastes for the military or represent better job matches, then reenlistment effects obtained from the restricted sample of survivors should provide a stronger test of the program's effect on job stability.

6. Bivariate probit estimates

Table 3 displays the bivariate probit estimates. After instrumenting for endogenous program participation,[20] we find that program effects on turnover and reenlistment are larger than in the previous probit estimates. JROTC participation reduces turnover by 12–17 percentage points and improves reenlistment by 8.6 points. Turnover effects are larger for females than for males, whereas the reenlistment effect is not significant for women. The larger overall bivariate probit estimates are consistent with the hypothesis that JROTC recruits are negatively selected and have unobserved characteristics that make them more likely to separate early and less likely to reenlist. This conclusion is supported by the estimated correlation between the error terms of the participation and outcome equations (ρ), which is positive and significant in the attrition models, and negative in the reenlistment models.

Table 2
JROTC effects on turnover and reenlistment.

	All	Males	Females
Panel A. Job match			
12-Month turnover	−0.132	−0.134	−0.106
	(0.016)***	(0.018)***	(0.034)***
	[−0.035]	[−0.036]	[−0.030]
24-Month turnover	−0.116	−0.112	−0.101
	(0.015)***	(0.016)***	(0.031)***
	[−0.038]	[−0.036]	[−0.033]
36-Month turnover	−0.088	−0.081	−0.081
	(0.014)***	(0.016)***	(0.030)***
	[−0.031]	[−0.029]	[−0.030]
First term turnover	−0.094	−0.091	−0.075
	(0.014)***	(0.015)***	(0.030)**
	[−0.035]	[−0.034]	[−0.029]
Panel B. Job stability			
Reenlistment	0.144	0.140	0.132
(36 month stayers)	(0.017)***	(0.019)***	(0.037)***
	[0.055]	[0.053]	[0.051]

Notes: All regressions include demographics (age, gender, race, marital status, and number of children), AFQT scores, education, and cohort dummies. With the exception of 12- and 24-month attrition, the models also include dummies for military occupation (10 categories). Full results are presented in Table A1. The attrition regressions include 325,560 individuals (269,020 males and 56,540 females). The retention regressions include 207,825 individuals (172,484 males and 35,341 females).
Standard errors are in parentheses and marginal effects are in brackets.
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

7. Fixed effects estimates

In contrast to bivariate probit, fixed effects estimation does not depend on an outside source of variation to predict participation, but rather assumes that non-JROTC recruits from the same zip code are more appropriate controls than the average recruit in the sample. This would be true if recruits who lived in the same zip code are influenced by the same socioeconomic conditions when making their decisions on JROTC enrollment and on military enlistment, separation, and reenlistment.

Fixed effects logit estimates are presented in Table 4 (odds-ratios appear in brackets). JROTC graduates are 17–23% less likely to leave during the 4-year contract, with the smaller effects occurring later during the first term of service. The fixed effects estimates are smaller than the bivariate probit estimates, but are still consistent with negative selection of JROTC participants. With respect to long-term job stability, JROTC graduates are 22% more likely to reenlist than other recruits from the same zip code, which exceeds both the univariate and bivariate probit estimates.

[18] This method effectively reduces the sample to recruits who lived in zip codes with JROTC schools. Because most zip codes contain only one JROTC high school (operated by one of the four military branches), fixed effects estimates do not depend on branch-specific program variation.

[19] Full results are presented in Table A1.

[20] About 41,176 recruits in our sample (16%) lived in a zip code that contained a JROTC high school. In the first stage regressions predicting JROTC participation, *ZIPJROTC* has an estimated coefficient of 0.143 (s.e. = 0.039) when using the sample for the attrition models, and a coefficient of 0.175 (s.e. = 0.043) for the restricted sample of stayers used in the reenlistment and promotion models.

Table 3
Bivariate probit estimates of JROTC effects on turnover and reenlistment.

	All	ρ	Males	ρ	Females	ρ
Job match						
12-Month turnover	−0.517	0.18	−0.473	0.16	−0.505	0.18
	(0.137)***	(0.06)***	(0.161)***	(0.07)**	(0.256)**	(0.12)
	[−0.117]		[−0.108]		[−0.119]	
24-Month turnover	−0.464	0.15	−0.291	0.08	−0.631	0.24
	(0.128)***	(0.06)***	(0.154)*	(0.07)	(0.230)***	(0.11)**
	[−0.134]		[−0.089]		[−0.174]	
36-Month turnover	−0.489	0.18	−0.381	0.13	−0.542	0.21
	(0.123)***	(0.06)***	(0.141)***	(0.06)**	(0.237)**	(0.11)*
	[−0.157]		[−0.126]		[−0.175]	
First term turnover	−0.507	0.18	−0.423	0.15	−0.465	0.17
	(0.121)***	(0.05)***	(0.142)***	(0.06)**	(0.239)*	(0.11)
	[−0.174]		[−0.148]		[−0.164]	
Observations	253,815		209,548		44,267	
Job stability						
Reenlistment	0.228	−0.04	0.203	−0.03	0.023	0.05
(sample: 36 month stayers)	(0.107)**	(0.05)	(0.123)*	(0.06)	(0.199)	(0.10)
	[0.086]		[0.076]		[0.009]	
Observations	161,925		134,182		27,743	

Notes: All regressions include demographics (gender, race, marital status, number of children), AFQT scores, education, and cohort dummies. Reenlistment models also include dummies for military specialties (10 categories). Additionally, all regressions include the following county-level variables: unemployment rates and per-capita income (in log form) for each year from 1990 to 2007; the county population serving in the Armed Forces (in log form) based on the 1990 and 2000 Census; the population living below poverty level (in log form) from the 1990 and 2000 Censuses, and the percent of the population black, Hispanic, and American Indian from the 2000 Census. The instrument for JROTC is an indicator for whether any high school in the recruit's zip code offered JROTC. The zip code was missing or erroneous for 20% of the original sample; these recruits were excluded from the sample used in these regressions. About 41,176 recruits in our sample (16%) lived in a zip code that contained a JROTC high school. In the first stage regressions predicting JROTC participation, *ZIPJROTC* has an estimated coefficient of 0.143 (s.e. = 0.039) when using the sample for the attrition models, and a coefficient of 0.175 (s.e. = 0.043) for the restricted sample of stayers used in the reenlistment and promotion models.
Standard errors are in parentheses and are robust to within-county correlation. Marginal effects appear in brackets.
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

The JROTC turnover effect is substantially larger for women than for men in the bivariate probit and fixed effects estimates. The positive impact of JROTC on reenlistment is confined to males in the bivariate probit results, but is observed for both men and women in the fixed effects results.

Fixed effects estimation deals with the bias induced from unobserved attributes of the localities where JROTC recruits lived, which may affect decisions to join JROTC and to pursue military careers. Fixed effects methods recover the treatment effect for the typical JROTC recruit (the average treatment effect on the treated, or ATT), whereas

Table 4
Fixed effects logit estimates of turnover and reenlistment.

	All	*N* (zip codes)	Males	Females
Job match				
12-Month turnover	−0.260	252,937	−0.260	−0.288
	(0.033)***	(3773)	(0.037)***	(0.115)**
	[0.771]		[0.771]	[0.750]
24-Month turnover	−0.230	253,660	−0.222	−0.234
	(0.029)***	(3860)	(0.032)***	(0.084)***
	[0.795]		[0.801]	[0.792]
36-Month turnover	−0.185	253,782	−0.173	−0.176
	(0.027)***	(3877)	(0.030)***	(0.072)**
	[0.831]		[0.841]	[0.839]
First term turnover	−0.182	253,844	−0.172	−0.144
	(0.026)***	(3887)	(0.029)***	(0.066)**
	[0.834]		[0.842]	[0.866]
Job stability				
Reenlistment	0.205	161,708	0.194	0.214
(sample: 36 month stayers)	(0.032)***	(3791)	(0.037)***	(0.072)***
	[1.228]		[1.215]	[1.239]

Notes: All regressions include demographics (gender, race, marital status, number of children), AFQT scores, education, and cohort dummies. Reenlistment models also include dummies for military specialties (10 categories). The average number of individuals in the same zip code is 60, with a minimum of 2 and a maximum of 846. Standard errors are in parentheses; odds-ratios in brackets.
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

Table 5A
Estimates of the effect of JROTC on promotion outcomes.

	Panel A. Probit			Panel B. Bivariate probit			Panel C. Fixed effects logit		
	All	Males	Females	All	Males	Females	All	Males	Females
Promotion to Rank E4	−0.053	−0.055	−0.032	−0.335	−0.373	−0.083	−0.084	−0.064	−0.127
	(0.020)***	(0.023)**	(0.043)	(0.143)**	(0.152)**	(0.466)	(0.041)**	(0.047)	(0.090)
	[−0.015]	[−0.016]	[−0.010]	[−0.107]	[−0.119]	[−0.026]	[0.919]	[0.938]	[0.881]
Promotion to Rank E4	−0.214	−0.225	−0.177	−0.370	−0.487	0.014	−0.194	−0.236	−0.397
(controlling for entry rank)	(0.023)***	(0.026)***	(0.047)***	(0.147)**	(0.142)***	(0.412)	(0.045)***	(0.054)***	(0.099)***
	[−0.060]	[−0.062]	[−0.055]	[−0.110]	[−0.148]	[0.004]	[0.824]	[0.790]	[0.672]
Promotion to Rank E5	0.021	0.018	−0.012	−0.137	−0.305	0.315	0.041	0.014	−0.0001
	(0.022)	(0.024)	(0.052)	(0.133)	(0.128)**	(0.346)	(0.046)	(0.051)	(0.119)
	[0.003]	[0.003]	[−0.001]	[−0.020]	[−0.044]	[0.038]	[1.042]	[1.014]	[1.000]
Promotion to Rank E5	−0.218	−0.220	−0.223	−0.483	−0.614	−0.049	−0.341	−0.367	−0.367
(controlling for entry rank)	(0.023)***	(0.025)***	(0.054)***	(0.230)**	(0.237)***	(0.488)	(0.047)***	(0.052)***	(0.123)***
	[−0.030]	[−0.033]	[−0.018]	[−0.055]	[−0.071]	[0.004]	[0.711]	[0.693]	[0.693]
Observations	192,657	159,891	32,766	150,068	124,347	25,721	148,342	122,265	22,956

Notes: All regressions include demographics (gender, race, marital status, number of children), AFQT scores, education, cohort dummies, and dummies for military specialties (10 categories). The promotion samples include only individuals present in all four years, for which we can observe the pay-grade level in each time period.
Standard errors are in parentheses and marginal effects are in brackets. For Panel C, standard errors appear in parentheses and odds-ratios in brackets.
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

bivariate probit models identify the local average treatment effect (LATE) from recruits who join JROTC because local schools offer the program. The difference in the estimates suggests that students who participate in the program because it is offered in the local high school have better job match quality than the typical JROTC student. Fixed effects turnover estimates also may be smaller (than bivariate probit estimates) because they do not control for the potentially stronger military tastes of JROTC graduates. However, instrumenting individual participation with school offerings accounts for the possibility that JROTC recruits have stronger military propensity, since placement of JROTC units is not affected by an individual's preferences.

Both the ATT and the LATE estimates are important for evaluating overall program effects. The average participant appears to have a more stable career and better job match than the average non-participant; however, these effects are even stronger for marginal participants who would not have joined this program, had it not been for its availability at the school.

A supplementary analysis of official military separation codes for early leavers provides additional support for the claim that JROTC participants have more stable careers due to better job matches. Table A2 analyzes group differences in military separation codes. The first column represents the difference in sample means of 22 separation codes between the two groups, while the second and third columns condition these differences on demographics and education, respectively. The results show that JROTC recruits are significantly less likely to leave for reasons suggesting poor job matches, such as displaying behavioral problems, enlisting by lying on the application (fraudulent entry), transferring to a different military branch, or being discharged for drug or alcohol use. Also, they are less likely to be released early due to downsizing, which suggests they display better job performance than others. Finally, JROTC recruits are more likely to transfer from the enlisted ranks to an officer commissioning program, which indicates a stronger propensity for military careers.

Table 5B
Ordered probit estimates of grade at the end of 4 years.

	All	Males	Females
JROTC	−0.011	−0.015	−0.023
	(0.016)	(0.018)	(0.035)
Observations	192,564	159,815	32,749

Notes: All regressions include demographics (gender, race, marital status, number of children), AFQT scores, education, cohort dummies, and dummies for military specialties (10 categories).
Standard errors are in parentheses.
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

8. Career progression

To investigate productivity effects, we analyze promotion to grades E4 and E5 during the first term of service. Because JROTC participants enter the military in advanced pay grades we estimate promotion models with and without conditioning on entry grade.[21] The promotion samples include individuals who survive for four years, so that career progression throughout the first term can be observed. Promotion results are presented in Tables 5A and 5B.

[21] It should be noted that our promotion indicators (promote to E4; promote to E5) measure whether the recruit reached a certain milestone by the end of the first term. Since all recruits have the opportunity to advance to these grades in the first term, regardless of entry level, these indicators do not favor JROTC recruits who enter in higher pay grades. Entering in an advanced pay grade may help reach these goals faster, but we are not concerned with the timing of promotion. Conditioning on entry pay grade aims to take into account any advantages that JROTC recruits may have in reaching the advanced pay grades.

Table 6
The effects of Navy-JROTC on turnover, reenlistment, and promotion.

	Probit			Bivariate probit			Fixed effects logit		
	All	Males	Females	All	Males	Females	All	Males	Females
Panel A. Job match									
12-Month turnover	−0.120	−0.153	0.006	−0.859	−1.101	−0.145	−0.073	−0.147	0.039
	(0.034)***	(0.039)***	(0.074)	(0.264)***	(0.280)***	(0.684)	(0.093)	(0.107)	(0.257)
	[−0.029]	[−0.037]	[0.001]	[−0.194]	[−0.250]	[−0.032]	[0.930]	[0.863]	[1.039]
24-Month turnover	−0.111	−0.142	0.006	−0.943	−1.060	−1.471	−0.096	−0.174	0.009
	(0.032)***	(0.036)***	(0.069)	(0.294)***	(0.288)***	(0.051)***	(0.082)	(0.094)*	(0.225)
	[−0.034]	[−0.043]	[0.002]	[−0.264]	[−0.300]	[−0.395]	[0.909]	[0.840]	[1.009]
36-month turnover	−0.120	−0.134	−0.069	−0.922	−1.033	−1.238	−0.091	−0.146	−0.063
	(0.030)***	(0.034)***	(0.066)	(0.253)***	(0.265)***	(0.519)**	(0.076)	(0.087)*	(0.218)
	[−0.042]	[−0.046]	[−0.024]	[−0.292]	[−0.326]	[−0.364]	[0.913]	[0.864]	[0.939]
First term turnover	−0.097	−0.110	−0.050	−0.911	−1.035	−1.241	−0.031	−0.104	0.109
	(0.030)***	(0.033)***	(0.065)	(0.245)***	(0.279)***	(0.908)	(0.073)	(0.083)	(0.204)
	[−0.035]	[−0.040]	[−0.018]	[−0.308]	[−0.347]	[−0.393]	[0.970]	[0.901]	[1.116]
Observations	9347	7310	2037	7460	5840	1620	6400	4796	1048
Panel B. Job stability									
Reenlistment	0.044	0.081	−0.067	−0.010	−0.081	1.409	0.047	0.132	−0.015
	(0.036)	(0.040)**	(0.079)	(0.386)	(0.446)	(0.055)***	(0.095)	(0.112)	(0.276)
	[0.016]	[0.030]	[−0.025]	[−0.004]	[−.030]	[0.442]	[1.048]	[1.141]	[0.985]
Observations	6276	4894	1382	5039	3925	1114	4049	2965	647
Panel C. Promotion									
To E4	0.064	0.058	0.102	0.191	0.503	1.003	0.160	0.116	0.408
	(0.043)	(0.049)	(0.097)	(0.370)	(0.520)	(1.412)	(0.123)	(0.147)	(0.350)
	[0.016]	[0.015]	[0.030]	[0.049]	[−0.121]	[0.167]	[1.174]	[1.123]	[1.504]
To E4	0.100	0.102	0.122	0.266	0.497	1.144	0.173	0.120	0.547
(controlling for entry grade)	(0.047)**	(0.054)*	(0.102)	(0.465)	(0.497)	(0.758)	(0.137)	(0.170)	(0.389)
	[0.024]	[0.023]	[0.034]	[0.062]	[−0.108]	[0.202]	[1.189]	[1.127]	[1.727]
To E5	0.104	0.148	−0.146	0.465	0.868	−0.414	0.285	0.411	−0.511
	(0.047)**	(0.051)***	(0.121)	(0.446)	(0.428)**	(0.640)	(0.139)**	(0.158)***	(0.591)
	[0.017]	[0.034]	[−0.020]	[0.084]	[0.118]	[−0.005]	[1.330]	[1.509]	[0.600]
To E5	0.092	0.141	−0.173	0.342	0.865	−0.672	0.277	0.400	−1.157
(controlling for entry grade)	(0.047)*	(0.052)***	(0.124)	(0.433)	(0.398)**	(0.479)	(0.140)**	(0.160)**	(0.716)
	[0.015]	[0.031]	[−0.021]	[0.057]	[0.114]	[−0.006]	[1.320]	[1.492]	[0.314]
Observations	5904	4619	1212	4745	3696	1049	3292	2341	544

See notes to Tables 2–4 for model specifications. The sample is restricted to JROTC recruits and the control group contains non-Navy JROTC recruits. For the bivariate probit regressions, the instrument used is "any Navy-JROTC unit in zip code". In all first stage regressions, this variable is highly significant. Standard errors are in parentheses. For probit and bivariate probit, marginal effects appear in brackets. For fixed effects estimates, odds ratios are in brackets.

* Significant at 10% level.

** Significant at 5% level.

*** Significant at 1% level.

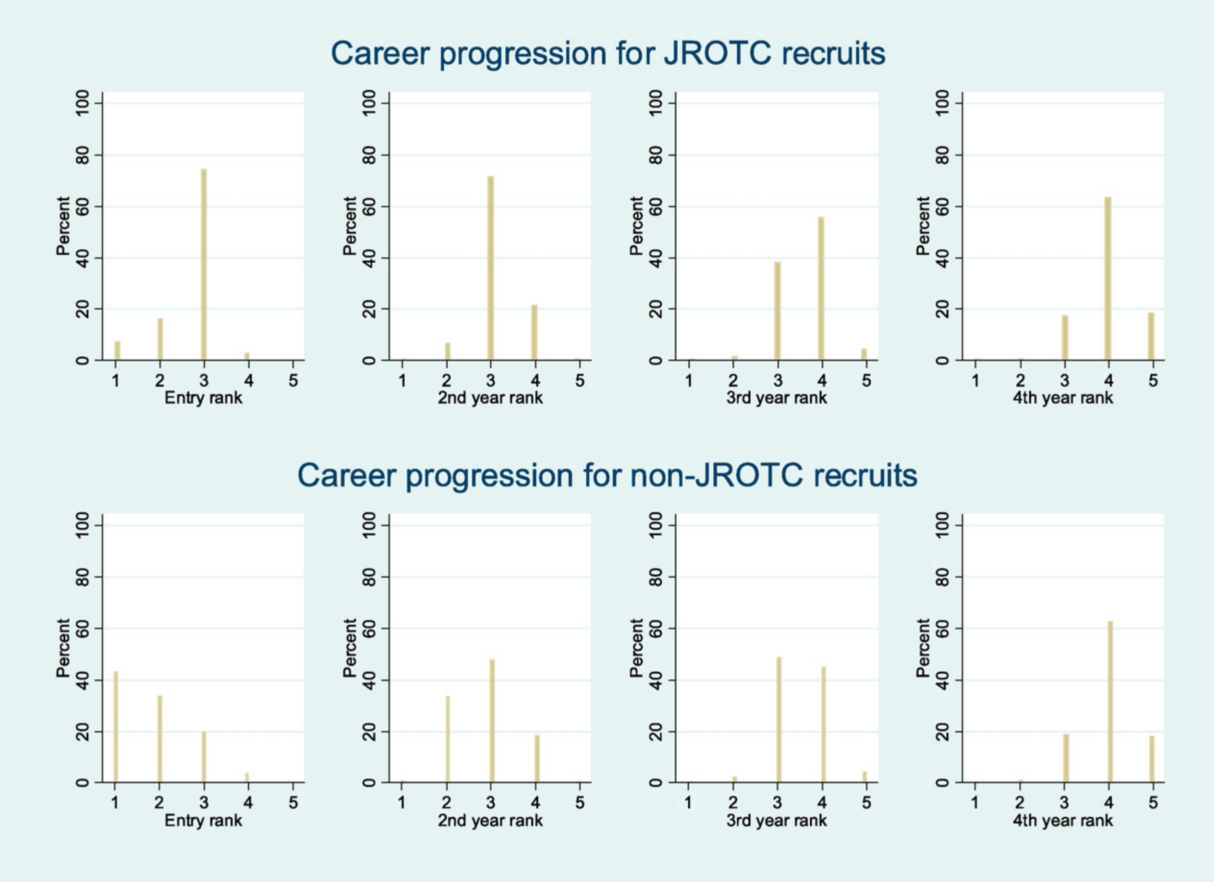

Fig. 1. The distribution of JROTC and non-JROTC recruits across grades during 4 years of service.

In Panel A of Table 5A, the probit estimates indicate that JROTC recruits are slightly less likely to achieve E4, and that the effect is larger when entry grade is held constant. JROTC recruits are just as likely to attain E5 as their peers, except when they are compared to recruits who enter in advanced grades. In Panels B and C, the bivariate probit and fixed effects results tend to confirm the probit results, although program effects are somewhat smaller. It appears that, although JROTC recruits enter in higher grades, other recruits overtake them by the end of the first term. To provide further evidence on promotion outcomes, Table 5B displays results from an ordered probit model of grade level at the end of four years. The results indicate that the grade distribution at the end of the first term is no different statistically between JROTC and other recruits. Similarly, Fig. 1 compares the progression of JROTC recruits and non-JROTC recruits and confirms that the positions of the two groups in the hierarchy converge at the end of four years of service.[22]

So far, the overall results suggest that vocational training primarily affects job-related outcomes by increasing job match quality rather than by enhancing human capital. In the military, wages are rigidly tied to grades, so our promotion results imply minimal wage differences during the first term. These findings may help explain why some prior studies find positive long-run wage effects of vocational education, but no such effects in the short run. If vocational education works by improving occupational selection, rather than by increasing occupation-specific human capital, wage differences in the short run may be negligible, only to become apparent later in the life cycle, as vocational graduates accumulate more occupation- or firm-specific human capital.

9. Specificity of vocational training

The evidence above supports the hypothesis that vocational education mainly improves job match quality. An

[22] One potential explanation for the absence of a promotion effect is that military promotion rules include a time-in-service requirement. The time-in-service requirement for promotion to E4 is 2 years, which means that new hires entering in grade E1 can overtake those who enter in grade E3 within 2 years. However, we focus on the probability of attaining a given rank by the end of the 4-year contract, rather than speed of promotion, which could be spuriously affected by entering at an advanced grade.

alternative hypothesis, discussed earlier, is that vocational education imparts occupation-specific skills but only for those whose occupation matches their vocational training (human capital effect). Our data allows us to distinguish between these two hypotheses.

JROTC offers a continuum of skill specificity ranging from general military knowledge to branch-specific skills. Each high school JROTC program is operated by one military branch and includes both general military courses as well as classes relevant solely to that branch. Therefore, in this section we separate JROTC recruits into those who complete Navy JROTC (NJROTC) versus those who complete a non-Navy, other JROTC program (OJROTC). By comparing the job performance of these two groups, we can shed light on whether the problem with finding positive returns to vocational education hinges on the definition of a training–occupation match. If JROTC mainly performs a sorting function by helping students discover whether they are well-matched with the military, the returns to JROTC should be the same regardless of curriculum. Alternatively, if the Navy-specific knowledge acquired in NJROTC is relevant solely to job performance in the Navy, then career outcomes for NJROTC recruits should be superior to other JROTC graduates.

This comparison should also reveal whether our previous estimates of the returns to JROTC are truly causal. For this analysis we include only JROTC recruits in the sample. Since students who participate in JROTC do so for similar reasons, and since they all enter at the same grade, comparing NJROTC to OJROTC allows us to hold constant institutional features and any further selection into the program that may confound our estimates. Results appear in Table 6.

Our probit results confirm that NJROTC recruits have lower turnover than OJROTC recruits. This result is driven largely by males in the sample, since turnover among women does not vary by curriculum. In our bivariate probit estimates, we use an instrument based on the presence of a school in the zip code that specifically offered NJROTC. This variable should be correlated with participation in NJROTC, but should not be correlated with performance. Based on this model, NJROTC reduces turnover for both men and women (for the latter only in some specifications), and increases retention for women. In contrast, fixed effects estimates indicate only a small turnover advantage among NJROTC men.

With respect to promotion, the promotion results indicate that, among men, NJROTC recruits have higher E5 promotion rates than OJROTC recruits. This result confirms the hypothesis that job performance is enhanced more by specific occupational training than by general vocational training. This evidence also suggests that returns to vocational education may hinge on the definition of the training–occupation match. Indeed, prior studies that use more detailed occupational categories do find positive returns to vocational education (Neuman & Ziderman, 1999), whereas those that use broad occupational categories (Hotchkiss, 1993) find no effects.

10. Conclusions

This study contributes to the debate on the benefits of high school vocational education by investigating the channels via which such programs influence careers. We address the following questions: Does vocational education directly enhance occupation-specific human capital or does it provide *ex ante* job information that helps trainees sort into occupations? Do more specific occupational skills impart additional benefits above and beyond those of general vocational training? Are vocational training effects homogeneous across male and female participants? While the literature focuses on wages as an indicator of labor market success, we investigate the effect of vocational training on early job turnover, job stability, and promotion in an internal labor market.

Using a rich data set of Navy recruits who received military science training via high school JROTC, we find that vocational education reduces early turnover and increases long-term job attachment. These effects are increasing in the specificity of the vocational skills, since graduates of Navy-JROTC programs have better job match quality than recruits from non-Navy programs. Women appear to benefit more from general information on military careers rather than from Navy-specific training. Also, the benefits of the vocational training are more pronounced for marginal participants (those who undertake such training because it is available at the high school), rather than the typical student who participates in JROTC.

Overall, the results suggest that one important effect of vocational training is to improve job match quality. Interestingly, vocational training also appears to improve job productivity, measured by promotion, only when we employ a narrower specification of the training–occupation match. This evidence helps reconcile the seemingly contradictory findings in prior studies. In particular, general vocational education may increase life-time earnings by improving job match quality and thereby fostering long-term employment relationships. Occupation-specific training also can have a positive human capital effect, subject to the researcher's ability to identify the occupation–training match in sufficient detail.

One benefit of lower turnover is to increase the organization's incentives to invest in further firm-specific training. Firms with less rigid internal policies than the military may offer more firm-specific training to vocational graduates than to other new hires, which would further contribute to long-term earnings growth of vocational graduates. Future research should investigate the extent and type of on-the-job training offered by employers as an additional channel via which vocational training may affect labor market success.

Appendix A.

Tables A1 and A2

Table A1
Full probit results of JROTC effects on turnover and reenlistment.

	12-Month turnover	24-Month turnover	36-Month turnover	First term turnover	Reenlistment
JROTC	−0.132	−0.116	−0.088	−0.094	0.144
	(0.016)***	(0.015)***	(0.014)***	(0.014)***	(0.017)***
	[−0.035]	[−0.038]	[−0.031]	[−0.035]	[0.055]
Female	0.066	0.053	0.064	0.073	−0.067
	(0.007)***	(0.006)***	(0.006)***	(0.006)***	(0.008)***
	[0.019]	[0.018]	[0.024]	[0.028]	[−0.026]
African American	−0.175	−0.140	−0.110	−0.114	0.295
	(0.007)***	(0.007)***	(0.006)***	(0.006)***	(0.008)***
	[−0.047]	[−0.046]	[−0.039]	[−0.043]	0.111
Hispanic	−0.260	−0.269	−0.266	−0.257	0.093
	(0.009)***	(0.008)***	(0.008)***	(0.008)***	(0.009)***
	[−0.067]	[−0.085]	[−0.092]	[−0.095]	0.036
Native American	0.032	0.043	0.048	0.043	0.030
	(0.015)**	(0.015)***	(0.014)***	(0.014)***	(0.018)
	[0.009]	[0.015]	[0.018]	[0.016]	[0.012]
Asian	−0.430	−0.475	−0.483	−0.490	0.325
	(0.014)***	(0.013)***	(0.012)***	(0.012)***	(0.013)***
	[−0.101]	[−0.137]	[−0.156]	[−0.169]	[0.120]
Other race	−0.180	−0.173	−0.164	−0.180	0.101
	(0.028)***	(0.026)***	(0.025)***	(0.025)***	(0.030)***
	[−0.047]	[−0.055]	[−0.058]	[−0.067]	[0.039]
Single with children	0.215	0.208	0.195	0.181	0.155
	(0.011)***	(0.011)***	(0.011)***	(0.011)***	(0.015)***
	[0.066]	[0.073]	[0.074]	[0.071]	[0.059]
Married, no children	0.197	0.110	0.049	0.047	0.033
	(0.069)***	(0.067)*	(0.066)	(0.065)	(0.084)
	[0.060]	[0.038]	[0.018]	[0.018]	[0.013]
Married with children	0.135	0.091	0.063	0.049	0.246
	(0.011)***	(0.011)***	(0.011)***	(0.011)***	(0.014)***
	[0.040]	[0.031]	[0.023]	[0.019]	[0.092]
AFQT	−0.005	−0.005	−0.005	−0.005	0.005
	(0.0001)***	(0.0001)***	(0.0001)***	(0.0001)***	(0.0001)***
	[−0.002]	[−0.002]	[−0.002]	[−0.002]	[0.002]
No high school diploma	0.437	0.497	0.513	0.494	−0.042
	(0.012)***	(0.012)***	(0.012)***	(0.012)***	(0.017)**
	[0.143]	[0.185]	[0.199]	[0.195]	[−0.016]
GED	0.369	0.426	0.451	0.440	−0.048
	(0.009)***	(0.009)***	(0.009)***	(0.009)***	(0.013)***
	[0.117]	[0.156]	[0.174]	[0.173]	[−0.019]
Some college	0.230	0.262	0.282	0.266	−0.013
	(0.014)***	(0.013)***	(0.013)***	(0.013)***	(0.018)
	[0.071]	[0.094]	[0.107]	[0.104]	[−0.005]
College	0.056	0.146	0.164	0.159	−0.121
	(0.019)***	(0.018)***	(0.017)***	(0.017)***	(0.022)***
	[0.016]	[0.051]	[0.062]	[0.062]	[−0.047]
Observations	325,560	325,560	325,560	325,560	207,825

Notes: Results obtained via probit. All regressions also include cohort dummies and ten occupational categories. Standard errors are in parentheses and marginal effects are in brackets.

* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

Table A2
Differences in separation codes.

Official separation code	JROTC vs. non-JROTC recuits		
	Difference in means	Controlling for demographics	Controlling for demographics and education
Desertion	0.0001	0.0001	0.0001
	(0.001)	(0.001)	(0.001)
Drugs	−0.013***	−0.014***	−0.013***
	(0.002)	(0.002)	(0.002)
Character or behavior disorder	−0.004**	−0.002	−0.002
	(0.002)	(0.002)	(0.002)
Discreditable incidents, civilian or military	0.002	0.000	0.001
	(0.002)	(0.002)	(0.002)
Substandard performance of duty	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)

Table A2 (*Continued*)

Official separation code	JROTC vs. non-JROTC recuits		
	Difference in means	Controlling for demographics	Controlling for demographics and education
Commission of a serious offense	0.003*	0.002	0.002
	(0.002)	(0.002)	(0.002)
Expiration of term of service (involuntary discharge)	0.006***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
Fraudulent entry, or erroneous enlistment	−0.016***	−0.017***	−0.016***
	(0.003)	(0.002)	(0.002)
Parenthood	0.002**	0.000	0.000
	(0.001)	(0.001)	(0.001)
Disability	−0.0001	−0.001	−0.000
	(0.002)	(0.002)	(0.002)
Unqualified for active duty	0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
Failure to adapt to military	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Failure to meet minimum retention requirements	0.002***	0.002**	0.002**
	(0.001)	(0.001)	(0.001)
Alcoholism	−0.002**	−0.001*	−0.001
	(0.001)	(0.001)	(0.001)
Expiration of term of service (voluntary discharge)	0.001	0.002	0.002
	(0.002)	(0.002)	(0.002)
Early or voluntary release, to attend school	−0.004***	−0.003***	−0.003***
	(0.001)	(0.001)	(0.001)
Dependency or hardship	0.0001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Discharge in lieu of court-martial	−0.002	−0.002*	−0.002
	(0.001)	(0.001)	(0.001)
Entry into officer commissioning program	0.001	0.002**	0.003***
	(0.001)	(0.001)	(0.001)
Involuntary transfer to another service upon completion of duty	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Voluntary transfer to another service upon completion of duty	−0.024***	−0.014***	−0.018***
	(0.004)	(0.004)	(0.004)
Early release due to downsizing, decommissioning, etc.	−0.004***	−0.003***	−0.003***
	(0.001)	(0.001)	(0.001)

Note: All results obtained via linear probability models with robust errors (in parentheses).
* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

References

Asch, B., & Hosek, J. (2007). The new economics of manpower in the post-cold war era. In *Handbook of defense economics, Vol. 2*. Amsterdam: Elsevier.

Bishop, J. (1989). Occupational training in high school: When does it pay off? *Economics of Education Review*, *8*, 1–15.

Bishop, J., & Mane, F. (2004). The impacts of career technical education in high school on labor market success. *Economics of Education Review*, *23*, 381–402.

Buddin, R. (2005). *Success of first-term soldiers*. Santa Monica, CA: Rand Corporation.

Buddin, R. (1984). *Analysis of early military turnover behavior*. Santa Monica, CA: Rand Corporation.

Coumbe, A., Kotakis, P., & Gammell, A. (2008). *History of the U.S. Army Cadet Command: Second ten years, 1996–2006*. Fort Monroe, VA: U.S. Army Cadet Command.

Elliott, M., Hanser, L., & Gilroy, C. (2002). *Evidence of positive student outcomes in JROTC-Career Academies*. Santa Monica, CA: Rand Corporation.

Hanser, L., & Robyn, A. (2000). *Implementing high school JROTC career academies*. Santa Monica, CA: Rand Corporation.

Hosek, J., & Sharp, J. (2001). *Keeping military pay competitive*. Santa Monica, CA: Rand Corporation.

Hotchkiss, L. (1993). Effects of training, occupation, and training–occupation match on wages. *Journal of Human Resources*, *28*(3), 482–496.

Jovanovic, B. (1979). Job matching and the theory of turnover. *Journal of Political Economy*, *87*, 972–990.

Klein, S., Hawes-Dawson, J., & Martin, T. (1991). *Why recruits separate early*. Santa Monica, CA: Rand Corporation.

Laurence, J., & Estrada, A. (2003). *A comprehensive study of the junior reserve officer training corps program*. Monterey, CA: Naval Postgraduate School.

Levesque, K., Lauen, D., Teitelbaum, P., Alt, M., & Librera, S. (2000). *Vocational education in the United States: Toward the year 2000*. Washington, DC: U.S. Department of Education, National Center for Education Statistics.

Levesque, K., Laird, J., Hensley, E., Choy, S., & Cataldi, E. (2008). *Career and technical education in the U.S., 1990–2005*. Washington, DC: U.S. Department of Education, National Center for Educational Statistics.

Levesque, K. (2003a). *Public high school graduates who participated in vocational/technical education: 1982–1998*. Washington, DC: U.S. Department of Education, National Center for Education Statistics.

Levesque, K. (2003b). *Trends in high school vocational/technical coursetaking: 1982–1998*. Washington, DC: U.S. Department of Education, National Center of Educational Statistics.

Meer, J. (2007). Evidence on the return to secondary vocational education. *Economics of Education Review*, *26*, 559–573.

Neuman, S., & Ziderman, A. (1999). Vocational education in Israel: Wage effects of the vocational education–occupation match. *Journal of Human Resources*, *34*(2), 407–420.

Neuman, S., & Ziderman, A. (1991). Vocational schooling, occupational matching, and labor market earnings in Israel. *Journal of Human Resources*, *26*(2), 256–281.

Neumann, G., Olitsky, N., & Robbins, S. (2009). Job congruence, academic achievement, and earnings. *Labour Economics*, *16*, 503–509.

Neumark, D. (2009). *Alternative labor market policies to increase economic self-sufficiency. Working Paper 14807*. Cambridge, MA: National Bureau of Economic Research.

Neumark, D. (2002). Youth labor markets in the United States: Shopping around vs. staying put. *Review of Economics and Statistics*, *84*(3), 462–482.

Neumark, D., & Rothstein, D. (2006). School-to-career programs and transitions to employment and higher education. *Economics of Education Review*, *25*(4), 374–393.

Pema, E., & Mehay, S. (2010). The impact of the high school junior ROTC program: Does treatment timing and intensity matter? *Defence and Peace Economics*, *21*(3), 229–247.

Pema, E., & Mehay, S. (2009). The effect of high school JROTC on student achievement, educational attainment, and enlistment. *Southern Economic Journal*, *76*(4), 533–552.

Polachek, S., & Robst, J. (1998). Employee labor market information: Comparing direct world of work measures of workers' knowledge to stochastic frontier estimates. *Labour Economics*, *5*, 231–242.

Rodgers, W., & Spriggs, W. (2002). Accounting for the racial gap in AFQT scores. *Industrial and Labor Relations Review*, *55*(3), 533–541.

Taylor, W. (1999). *Junior reserve officers' training corps: Contributing to America's communities*. Washington, DC: Center for Strategic and International Studies.

U.S. Department of Education. (1987). *High school vocational training*. Washington, DC: Center for Educational Statistics.

U.S. Department of Education. (2004). *National assessment of vocational education: Final report to Congress*. Washington, DC: U.S. Department of Education.

Warner, J., & Asch, B. (2001). The record and prospects of the all-volunteer military in the U.S. *Journal of Economic Perspectives*, *15*(2), 169–192.

Williamson, S. (1999). *A description of U.S., enlisted personnel promotion systems*. Santa Monica, CA: Rand Corporation.

Yates, J. (2005, February). The transition from school to work: Education and work experiences. *Monthly Labor Review*, 21–32.